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sandstone; this lava was partially denuded, and buried under a conglomerate composed of its *débris*, mingled with rhyolitic, trachytic, and granitic material. The detritus was also buried under another lava-flow; and this alternating action went on, first with increasing and then with diminishing eruptive activity, until the western sandstones and conglomerates were reached, which were laid down on the last lava-flow. It is probable the lava came from fissure eruptions. Wherever the detritus was deposited on the lava, whether within the trappean belt or on its western side, denudation has taken place, and fragments of the trap (melaphyr and diabase) have been enclosed in the overlying detritus. Unconformability would, of course, thus exist, and the writer has figured such a case; but it is the unconformability that always exists when lava flows on a shore, and is subjected to the denuding action of the waves, and proves nothing regarding the geological age.

The evidence which Irving claims has been ignored, and which he says is "proof absolute that the Keeweenaw [copper-bearing rocks] series belongs below the base [Potsdam] of the paleozoic column of the Mississippi" (*Geol. Wisc.*, iii. 23), is principally the finding of a trappean rock at Taylor's Falls, against which rest sandstone and shales holding fragments of the trap and primordial fossils. Excepting the fossils, these are exactly the conditions which are found, and which ought to be found, within the copper-bearing belt, and on its western side; and it proves nothing regarding geological age, but only sequence of time. If such evidence as this is 'proof absolute' of distinct geological age, then there is proof absolute that there are as many different geological formations in the copper-bearing rocks as there are detrital beds enclosed in the traps, and proof that the last lava-flow of any active volcano, reaching the sea, is separated by a distinct age and 'immense unconformity' from the detritus deposited upon it before it is hardly cold. Unconformity of itself proves nothing, unless both formations are sedimentary; for an eruptive rock cannot, from the very nature of the case, be conformable, in the true sense, with any thing. The relations that the old basaltic lavas have, according to Irving, to the western sandstone, are exactly what they ought to have from their origin, as shown thirty-three years ago.

Again: according to the Wisconsin geologists, the Taylor's-falls trap is fifteen miles from any other so-called copper-bearing rocks, and may as well be an azoic rock; for similar ones have been collected by the writer in the granite of the Marquette azoic district. If it is referred to the copper rocks on lithological grounds, the same argument could be used to unite with this series a large part of the basaltic traps the world over. The resemblance between them is, in the writer's opinion, that which any two basaltic lava-flows or dikes have wherever they may have been extruded.

The writer has shown that the first trap on the east overflowed and indurated the eastern sandstone; and he collected specimens showing the induration, the trap, and the trappean detritus in the overlying conglomerate. Therefore Irving's statements, that the eastern sandstone unconformably overlies the trap, and that no trappean detritus occurs in the fragmental rocks, are incorrect; and the published evidence was in his hands several years ago. Irving is mistaken when he says that all the geologists who approached the question from the east felt baffled, as the writings of Foster and Whitney, Selwyn, or myself, give no indications of the kind. It may be mentioned, that in 1850 Foster and Whitney showed that a fault

existed along part, at least, of the eastern side of the traps, and that the Bohemian range was a later protrusion. This evidence will explain the apparent unconformity of the traps with the eastern sandstone observed in some places.

For a fuller discussion of the copper-bearing rocks and allied formations, together with the literature down to 1880, the writer would refer to the bulletin of this museum, vol. vii. pp. 1-157.

M. E. WADSWORTH.

Museum of comp. zool., Cambridge,
Mass., March 15, 1883.

Domestic ducks that fly abroad like pigeons.

In response to Mr. Storer's note under the above heading (*SCIENCE*, No. 3), I would state that in my boyhood I lived on a plantation in Liberty County, Ga., on which there were a great many domesticated ducks, both mallards and musk-ducks. Many of these latter belonged to the negroes, and were tended with but little care. Near by the negro village there was a swamp full of large trees, and often covered with water. A considerable portion of the swamp was cleared, and annually planted in rice; but many dead cypress (*Taxodium*) trees still remained standing. This swamp was a favorite resort for wild ducks of all kinds, especially mallards, teal, and summer ducks (wood-ducks). Many domesticated musk-ducks, especially those belonging to the negroes, flew abroad every morning, remained in the swamp (one to two miles distant) all day, and returned at night. Some of them built their nests and reared their young in the swamp, though they never became thoroughly wild.

I never observed this habit, except in the musk-duck. The reason, I think, is plain. In shape, in gait, in flight, and in habits, the musk-duck is very similar to the wood-duck (sponsa). Like the latter, it walks with freer step, it rises, flies, and alights with greater ease and grace, than other species, because the wings are broader and rounder. Like the wood-duck, also, it alights on trees. The dead cypress-trees were a favorite resting-place for the musk-ducks. Like the wood-duck, too, it builds its nest on trees or stumps, and takes down the young when hatched. I have never known the musk-duck to build on the tops of tall cypresses, like the wood-duck, but often on the tops of hollow stumps fifteen to twenty feet high.

JOSEPH LECONTE.

Berkeley, Cal., March 15.

Apparent attractions and repulsions of small floating bodies.

To obviate possible misunderstandings, it may be proper for me to make a few remarks in relation to 'E. H. H.'s' *critique* (*SCIENCE*, i., p. 43) on my article (*Amer. Journ. sc.*, Dec., 1882) on the above phenomena.

I am to blame for whatever ambiguity attaches to the use of the term 'tension' as applied to the explanation of these phenomena. In one instance (that cited) I inadvertently used the expression 'superior tension' instead of 'superior force.' But inasmuch as in the formal announcement of the capillary principle — which is applied to the case in question, and also in the preceding as well as the succeeding context — it is very clearly indicated that the effective capillary forces (and not the *surface-tension*) are regarded as inversely proportional to the radii of curvature of the menisci, few physicists will, I trust, be misled by the expression.

He does not admit "that a liquid film tends to draw a solid, to which it is attached, toward the centre